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A NOTE ON THE PROBLEM OF INDUCTION

THE so-called "problem of induction" comprises many different but related questions. This variety is in part due to the difficulty of finding a satisfactory solution: older formulations are given up and are replaced by weaker problems in the hope that what is impossible to prove in the strong case might perhaps yield to proof in a weaker case. Roughly, the development is as follows.

Originally it was believed that the conjunction $P(a_1)\&P(a_2)\cdots\&P(a_n)$, or $P(n)$ for short, could in some way guarantee the *truth* of $(x)P(x)$. [The predicate 'P' occurring here may be expressed in ordinary English ("being black, provided one is a raven"); it may be expressed in terms of physics ("moving on a straight line with constant speed with no forces present"); or in terms of some other discipline. This way of defining P allows us to state any theory in the form $(x)P(x)$.] This assumption, which I shall call the *simple generalization hypothesis,* leads to the program: to discover, and to state explicitly, the specific inferences according to which $(x)P(x)$ can be obtained from $P(n)$. The hypothesis was refuted by Hume, who also showed that the corresponding program could not be carried out.

Next, the simple hypothesis was replaced by the assumption that $P(n)$ might guarantee a *high probability* (in the objective sense) of $(x)P(x)$. Hume's argument refutes this hypothesis also (the disproof was provided already by Hume himself).

The breakdown of both the simple and the probabilistic generalization hypotheses led to the search for a formulation of the key statement of the problem of induction which would be weak enough to escape refutation by Hume's arguments. The formulation which provides the background of much contemporary thought on the problem and which we shall call the *modified generalization hypothesis* asserts that, given $P(n)$, it is *reasonable* to adopt $(x)P(x)$. I shall now make a few comments on this modified hypothesis.

First, it is important to point out that this hypothesis is no longer concerned with the truth, or even with the probability (in

the objective sense) of the generalization whose use it recommends. It asserts that it is *reasonable* to generalize a predicate that has been found to be instantiated in a finite number of cases. It does *not* assert that the result of the generalization will be *true*, or even *highly probable*. It is particularly important to make this point. The modified hypothesis has often been misunderstood as justifying an expectation of *success*. Even the most careful thinkers are sometimes found to believe that proof of the modified hypothesis gives them the right to expect success, or success in the long run. This of course means regressing to the simple generalization hypothesis or to the probabilistic generalization hypothesis, which have both already been refuted.

Secondly, it should be realized that the modified hypothesis is behind almost all recent attempts to solve the problem of induction (with the sole exception of Popper's theory). All these attempts in one way or another aim to prove the hypothesis. This is certainly true of the argument that the modified hypothesis agrees with common sense. But it is also true of the more complex attempt to show that, given $P(n)$, $(x)P(x)$ has a higher degree of confirmation than any one of its alternatives [where by an alternative to $(x)P(x)$ we mean a statement that is as general as $(x)P(x)$ but entails $\overline{(x)}P(x)$]. The concentration upon $(x)P(x)$ and the neglect of alternatives is found even in Hume. One almost never starts with $P(n)$ and asks *what* generalization should be adopted. One *takes it for granted* that adopting $(x)P(x)$ is the right thing to do, and one looks for some plausible argument supporting this belief.

Thirdly, it is clear that the modified hypothesis has a much greater chance of succeeding than any one of its more demanding predecessors. What is reasonable or not is a notoriously vague affair, and it is easy first to define (either by explicit stipulation or by uncritical adoption of commonly accepted standards) a "reasonable procedure" in terms of direct generalization from finite evidence, and then to obtain the modified generalization hypothesis by an analysis of the procedure thus defined. If definition and analysis are separated by many steps, or by many years, then the circularity will not at all disturb; it will rather create the impression that a particularly solid foundation has been found for the hypothesis. It needs of course only little thought to realize that, circularity or no, the standards implied in common behavior are themselves open to criticism and that it is the task of the philosopher to provide such criticism, and not to be satisfied with popularity.

Assume, now, fourthly, that the modified hypothesis is *false;* that is, assume that given $P(n)$ it is *not* reasonable to choose $(x)P(x)$ over all its possible alternatives and that it *is* reasonable to consider at least some of those alternatives. Such a result would refute the simple generalization hypothesis and the probabilistic generalization hypothesis and would also be stronger than Hume's disproof of them. The first point follows from the fact that it is desirable, or reasonable, to choose what is known to be true or highly probable over what is known to be false or of low probability. For if it is *not* desirable, or reasonable, to choose $(x)P(x)$ over any one of its alternatives, then the truth or the high probability of $(x)P(x)$ cannot have been established, as this would mean that it was desirable to choose what is known to be false in addition to what is known to be true, and to choose what is known to have a low probability in addition to what is known to have a high probability. The second point becomes clear if we consider that Hume showed the *impossibility* of obtaining the truth or the high probability of $(x)P(x)$ given $P(n)$, whereas refutation of the modified generalization hypothesis would in addition show the *undesirability* of such an achievement. It would not only show that the problem of induction *cannot* be solved; it would also show that it *should not* be solved.

Having explained the role of the modified generalization hypothesis, I now proceed to show that this hypothesis can indeed be refuted and that the procedure suggested by it can be shown to be undesirable.

The argument will be in three steps. I shall first discuss abstractly an individual case of a rather peculiar nature. I shall then give an example that exhibits all the features described in the abstract discussion. Finally, I shall show that the defender of *any* theory must act as if his theory possessed these features. This final step will refute the modified generalization hypothesis.

The abstract discussion is as follows. Consider a theory T (expressed, as indicated in the beginning, by universalizing the property P) which entails that F. Assume that actually F' (where "F takes place" is inconsistent with "F' takes place"). Assume also that the laws of nature forbid the existence of equipment for distinguishing F and F'. The theory T is then obviously false; only we shall never be able to discover this by a consideration of "the facts" only.

An example that vividly illustrates this situation is the phenomenon of Brownian motion. This phenomenon refutes the second law of the phenomenological theory of thermodynamics:

the Brownian particle is a machine that achieves what the second law says should not occur. It absorbs heat from the surrounding fluid and transforms it directly into (its own) motion. Still, it is impossible to show in a direct way that a refutation is taking place. The reasons are as follows. It is impossible to follow in detail the path of the particle in order to detect the amount of work done against the fluid (this is connected with the fact that the Brownian motion obeys an uncertainty relation very similar to the relations known from quantum theory, the diffusion constant of the medium replacing the quantum of action). And it is also impossible to measure the amount of heat lost by the fluid (this is due to the fact that the fluctuations to be measured will inevitably be overlaid by the fluctuations of the thermometer. This "noise" depends on the temperature only and cannot be eliminated). Result: the Brownian motion refutes the second law, but the facts are such that they do not allow us to discover the violation. There are many other cases which show exactly the same features (an example would be the motion of an electron in the shell of an atom). What is the solution? To explain it, we return to the abstract argument.

Assume that, in addition to T, we introduce another theory T' [expressed by universalizing a property whose universalization entails $\overline{(x)}P(x)$] which covers the facts supporting T, makes successful additional predictions A, and entails that F'. The test of the additional predictions may be regarded as an indirect proof that F' and, thereby, as an indirect refutation of T.

In the case of our example it was Einstein who resolutely used the kinetic theory of matter (which plays the role of the alternative theory T') for calculating those properties A of the Brownian motion which can be checked by experiment (the main prediction was the proportionality between time and the mean-square displacement of the particle) and who thereby made an indirect refutation of the phenomenological theory possible.

It is clear that the existence of cases like the one we have just discussed cannot be ascertained in advance. *Any* theory T under consideration (including that zero-case of theory construction "all ravens are black") may be inconsistent with facts which are accessible only indirectly, with the help of an alternative T'. Now it is surely reasonable to demand that the class of refuting instances of a given theory be made as large as possible and that especially those facts which belong to the empirical content of the theory, which refute the theory but which cannot be distinguished from

similar, but confirming facts, be separated from the latter and be thus made visible. However, this means that, given $P(n)$, it is reasonable to use not only $(x)P(x)$ *but as many alternatives as possible.* This is the promised refutation of the modified generalization hypothesis.

As we have said above, this refutation demands of us a completely new attitude toward the so-called "problem of induction." The fact that the problem is so difficult to solve need not worry us any longer. As a matter of fact, we should rejoice that we are not restricted, by some proof, to the use [given $P(n)$] of *one* generalization only and are thus able to discover some perhaps decisive shortcomings of this generalization.

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HOW BELIEF IS BASED ON INFERENCE *

IF I believe p solely as a result of making a *conscious inference* from other propositions which I believe, then my belief in p is *based on* this conscious inference. If my belief in p is based on an inference i, then a necessary condition for my being warranted or justified in believing p, and also for my knowing p, is that the inference i be valid or warranted. If my belief in p is based on an inference i, then whether or not I am warranted in believing p, and whether or not I know p, depends to a large extent on the inference i. Philosophers have been interested in the inferences on which beliefs are based because they think that what a person knows depends on what he is warranted in believing and that this depends upon the inference on which his belief is based.

If the only beliefs that are based on inference are those beliefs which are held as a result of conscious inference, then few beliefs are based on inference. If this is so, then analysis of warranted belief, and of knowledge that takes belief to be based on inference, will have limited application.

I shall argue that beliefs may be based on inference even when they are not based on conscious inference. I shall argue also that it is possible for a philosopher to hold that all or most beliefs are based on inference. That is, I shall attempt to define a relevant meaning for the phrase 'based on inference' such that questions

* In writing this paper I have benefited from comments made by Paul Benacerraf and Richard Rorty.

about the warrant for a belief become questions about the warrant or validity of an inference.

1. *An inferential model for belief formation.* The words 'belief' and 'inference' each suffer from an ambiguity which we may elucidate by distinguishing believing and inferring (as things one "does") from an abstract belief (a proposition believed) and an abstract inference (a series of propositions composing the abstract inference).

To say that my belief in p is based on the (abstract) inference i is to claim that stating the inference gives the reason why I believe p, in some sense of 'reason'. Here one is torn between taking 'reason' as 'cause' and taking it as 'justification'. That is, sometimes I believe p *because* I consciously made the inference i; and this explains how my belief is based on inference. At other times my belief is not the result of conscious inference, although I would mention the abstract inference i if asked to *justify* or defend my belief in p; and now this explains how my belief is based on inference. Yet there are many cases in which my belief in p might be said to be based on an inference i even though I made no conscious inference and am also unable to express my reasons for believing p as an inference.

Furthermore, in stating the abstract inference i as part of my *reason* for believing p, it is not at all the case that 'reason' here means 'either the cause of my belief or the justification for it'. On the contrary, 'reason' seems to have the sense of 'both the cause of my belief and the justification for it'. This is clear enough where I make a conscious inference. Here the cause of my belief is my making the inference i, and furthermore the abstract inference i should be indicated in order to give my justification for my believing p. The matter is less clear where there is no conscious inference. The fact that I would refer to i if asked to defend my belief in p need not indicate that my belief is based on i. I might be lying, or rationalizing. My belief in p must somehow actually depend on i.

But what does it mean to say that my believing p depends (causally) upon something abstract, like an inference? Being abstract, the inference itself does not change over time. It itself cannot determine that I shall come to have a certain belief at a certain time.

What must be meant, then, is not that an abstract inference is the cause of my believing p, but rather that the specification of the cause in some way refers to an abstract inference. That is, one is imagining some correlation between particular types of causes

of coming to believe, on the one hand, and particular abstract inferences, on the other hand.

Let $C(x)$ be a (one-one) function that takes abstract inferences for arguments and has possible causes of believing as values. Then $C(x)$ represents a type of cause of coming to believe things such that each particular cause may be correlated with an abstract inference. To say that my belief in p is based on inference i involves two things: first, the hypothesis of the existence of a one-one function $C(x)$, representing a type of cause of coming to believe things, and satisfying a further condition described in the next paragraph; second, the claim that my believing p is or was caused by $C(i)$.

The further condition on $C(x)$ is this: If my believing p is caused by $C(i)$, then whether or not I am warranted in believing p depends for the most part on whether or not i is a valid or warranted inference.

I take it that this analysis accounts for the main part of what philosophers have meant by 'based on inference' when they have held that, in general and for the most part, belief is based on inference. It follows that such philosophers have been, or should have been, advocating a hypothesis about the formation of belief.

According to this hypothesis, there is a typical way in which beliefs are formed or caused. There is an analogy between the typical cause and conscious inferring. This analogy consists in the fact that we are able to correlate a particular cause $C(i)$ with an abstract inference i such that the warrant of the resulting belief depends on the warrant of the correlated inference. Because of this analogy, we may speak of the philosopher as urging an "inferential model" for belief formation.

2. *Why a warranted belief may be trusted.* I now want to describe the inferential model of belief formation from a slightly different point of view. Up to now I have been emphasizing the causal hypothesis involved in the claim that a belief is based on inference. But the other part of that claim is equally important: the notion that whether or not the belief in p is warranted depends on whether or not the corresponding inference is warranted.

When a belief is warranted, it can be trusted because of the way the belief came about. In other words, given that the believing in p came about the way it did, p is likely to be true. If a belief in p is warranted, then it should be reasonable to infer from the way the believing in p came about to the truth of p.[1] Therefore,

[1] Given that we have no further evidence concerning the truth or falsity of p.

if one is to understand what it is for a belief in p to be warranted, he must understand what the inference is from the fact that believing p came about in a certain way to the truth of p. How can we infer from the cause or explanation of someone's believing p to the truth of p?

In general, the causal explanation of someone's believing p will refer to two things: (1) previous beliefs of the person; let these beliefs be expressed by B; (2) certain events that "happen to" or "stimulate" the person; let these events or stimulations be described by an appropriately chosen E. Now, if we are to infer from the cause of the believing in p to the truth of p, then this will be because there are certain further assumptions we think warranted (let them be expressed by A) and appropriate descriptions E such that the inference from the conjunction of B, E, and A to the truth of p is a warranted inference. If this inference is warranted and if we believe the person was previously warranted in believing B, then we may trust p,[2] and he is warranted in believing p.

Let us suppose that the extra assumptions A remain constant. Then the causal explanation of various believings will normally involve mention of different prior beliefs B and further events E. Each particular cause (for each believing) may be correlated with a particular B and E and, therefore, with a particular inference from the conjunction of B, E, and A to the truth of p. Furthermore, this inference must be warranted if the believing of p is to be warranted.

Therefore, the account of how a belief may be warranted ties up with the earlier discussion of what it is for belief to be based on inference. Thus, the believing of p was caused by $C(i)$. $C(i)$ involves the fact that there are prior beliefs expressed by B and also the occurrence of the events described by E. Belief in p is based on the inference i, where i is the inference from the conjunction of B, E, and A to the truth of p. If the prior beliefs in B are warranted, then whether or not belief in p is warranted depends on whether or not the inference i is warranted.

3. *The inferential model in more detail.* I have explained one sense in which beliefs may be based on inference; and I have described how the claim that beliefs are generally based on inference may be treated as a hypothesis in learning theory (or, if you like, about learning theory). According to this hypothesis, the usual

[2] See note 1.

process of coming to have a belief p involves prior beliefs expressed by B and certain events described by E such that there are constant assumptions A such that the conjunction of B, E, and A serves as premise for a more or less warranted inference to p. Therefore, according to this hypothesis, there is a model for the process of coming to believe things, where the model is that of conscious inference.

What are the events described by E? Presumably these events will be the occurrence of certain sensory stimulation. Some philosophers will take such events to be the occurrence of nerve impulses; others will opt for psychological experience ("sense data"). E describes what some philosophers have taken to be the epistemological data. E, in other words, describes current experience taken in some basic way.

The prior beliefs B may have arisen previously in the same manner that the present belief p has arisen. But I do not mean to be committed to a traditional empiricism. The beliefs expressed by B may include innate beliefs.[3] A further departure from traditional empiricism consists in the recognition that in general the inference i involves more premises than B and E. These further assumptions A represent, as it were, an innate tendency of the mind and not just further innate beliefs.

A brief word here about my talk of innate beliefs and tendencies. First of all, I do not mean to suggest that it is improper to attempt an empirical explanation for innate beliefs and tendencies (using, e.g., evolutionary theory). Second, those who believe that the theory of learning will not need to rely on assuming innate beliefs and tendencies may still make use of the above analysis, by assuming that there will be no innate beliefs expressed by B and no further assumptions A. Disagreement here may be empirical, over the facts of learning, or conceptual, over what is to count as knowledge.[4]

To specify a particular theory of learning, then, involves the following matters: (1) One must specify what constitutes the data from experience and also how this experience is to be described in the description E. One must also decide whether E describes total experimental data or only some part, e.g., to which a person is "attending." (2) One must specify what beliefs are to be expressed by B. These beliefs are active, or causally relevant.

[3] Whether or not innate beliefs can be warranted depends on whether or not there can be innate knowledge and whether or not knowledge implies warranted belief.

[4] See note 3.

Ideally one wants to explain how it is that beliefs become "activated." (3) One must specify the extra assumptions A, if any. (4) Finally, one must specify those conditions K which must be satisfied if, given that a person has current experience described by E and has "activated" beliefs B, where there is a more or less good abstract inference from the conjunction of E, B, and A to p, this person is to come to believe p.

To believe that belief is generally based on inference is to believe that some theory of learning, theoretically specifiable as above, is true.

4. *Must there be beliefs not based on inference?* The main point of all this is that I hope to have shown how it is possible for a philosopher to hold that, in general, belief is based on inference. A further point follows: if one accepts my account of how a belief may be based on inference, then one may consistently hold that all beliefs are based on inference.

This further point is sometimes denied. It is sometimes argued that, if some belief is based on inference, then some belief must fail to be based on inference. Otherwise, it is argued, either there would have to be an infinite regress of a sort that cannot exist (presumably because it entails that we have an infinite number of different beliefs and this is taken to be absurd) or there would have to be a circle such that some beliefs are based on themselves (and this is again taken to be absurd).

Now whatever one thinks of such arguments, it is clear that they must depend upon the assumption that, if my belief is based on inference, then I must already have believed the premise of that inference. But, if the account of the inferential model given above is acceptable, my belief may be based on inference even when I do not believe all (or any) of the premises.

To make this clearer, let me define a *conscious inference* as a coming to believe in something where the inferential model applies, but where there is no E nor A but only B, and furthermore when one is aware that one is inferring. If a conscious inference is made, then one must previously have believed the premises of the inference. Similarly, an *unconscious inference* may be defined as a coming to believe for which there is no E nor A but only B, where this time one is not aware that he is making an inference. Here too, if an unconscious inference, in this sense, is made, then one must previously have believed the premises of the inference. In either conscious or unconscious inference, then,

premises must have been previously accepted; and this may lead some philosophers to think it a necessary feature of belief based on inference that the premises of the inference must have been previously believed.

But the inferential model applies (and therefore belief is based on inference) in many other cases. In these other cases it will not follow that premises were previously believed. In particular, consider a coming to believe to which the inferential model applies but for which there is no B. This will be a case in which the resultant belief depends only on the events or stimuli described by E (and perhaps on the innate tendency represented by the further assumptions A). Here a belief is based on inference without depending on other beliefs previously accepted. Such an example shows how the regress may be halted, the circle broken out of. Thus, it is not impossible that all beliefs be based on inference, in my sense. And it is possible for an epistemologist to take some beliefs to be warranted because they are based on warranted inference without assuming that some beliefs are warranted without being based on inference. Similarly, it is possible for an epistemologist to hold that there is no noninferential knowledge, if to be inferential is to be based on inference in my sense.

5. *Summary.* In this paper I have explained a sense of 'based on inference' such that to say that a belief in p is based on inference is to claim: (1) there is a type of cause of belief represented by the one-one function $C(x)$, taking abstract inferences for arguments and designating possible causes of belief as values; (2) $C(i)$ caused the believing in p; (3) $C(i)$ involves certain prior beliefs expressed by B and certain further events (or stimuli) described by some E such that (for some constant assumptions A) i is an abstract inference from the conjunction of B, E, and A to the truth of p; and (4) on the assumption that the believing of the prior propositions expressed by B is warranted, whether or not believing p is warranted depends upon whether or not the inference i is warranted. I have shown (in Section 2, above) how this account of 'based on inference' arises naturally from consideration of what it is for a belief to be warranted. If my account is accepted, it is not logically impossible that all beliefs be based on inference.

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INFERENCE, EXPLANATION, AND PREDICTION

I

ACCORDING to an influential but still controversial theory of explanation,[1] an explanation of an event is a "covering-law" or "deductive-nomological" argument, i.e., a deductive argument whose premises include general laws essentially. An event is said to be explained if an argument of this sort is constructed whose conclusion is a statement describing that event.

Consider the following covering-law argument:

(L) The adaptive response of animals to continued cold climatic conditions is such that they increase in size.
(C) Cold climatic conditions prevailed during the Pleistocene ice age in the regions inhabited by horses.

(E) Horses increased in size during the Pleistocene ice age.

What is this argument supposed to be? Of course it could be an explanation of the fact that the size of horses increased during the ice age. But this surmise is inconclusive: it could equally plausibly be construed as a retrodictive argument intended to establish this fact from the previously known general law (L) and previously ascertained singular statement (C). Further, if the argument is expressed in a tense-neutral language, it could just as plausibly be construed as a predictive argument intended to predict the increase in the size of horses on the basis of (L) and (C); for the only clue relevant in ruling out the argument as a predictive argument is the past tense of (E). Still further, the argument may be an exercise in deductive logic, for all we know.

This brings out two points: first, what we can call an explanation, a predictive, or a retrodictive argument is not an argument considered in an abstract way, but rather a concrete argument that is given at a certain time, in a specific context, and for a specific purpose. Another way of putting this would be to say that explanations, predictive, and retrodictive arguments are not *argument-types* or *inference-types,* but specific *argument-tokens* and *inference-tokens.* From this it obviously follows that statements occurring in a given explanation, predictive or retrodictive argument are also *statement-tokens* rather than *statement-types.*[2] Secondly, we see that in order precisely to characterize and distin-

[1] I refer of course to the so-called covering-law theory of explanation as presented, e.g., in C. G. Hempel and P. Oppenheim, "Studies in the Logic of Explanation," *Philosophy of Science,* **15** (1948).

[2] This point was made by I. Scheffler in "Explanation, Prediction and Abstraction," *The British Journal for the Philosophy of Science,* **7** (1957).

guish among explanatory, predictive, and retrodictive arguments, it is essential to have knowledge of the features of an argument that are not purely formal or logical. There may be nonlogical relationships between the premises and the conclusion that are relevant in distinguishing among explanatory, predictive, and retrodictive arguments. What are these nonlogical features and relationships? How are they relevant for a theory of explanation and prediction?

An argument-type conforming to the requirements of the covering-law model can be used as an explanation, a predictive argument, or a retrodictive argument, or for still other purposes. A purely logical and formal analysis such as that provided by Hempel and Oppenheim in their classic paper is incapable of showing whether a given argument is an explanation, a predictive, or a retrodictive argument; it cannot differentiate between these different scientific procedures. And as we shall see, there are important conceptual differences between explanation on the one hand and predictive and retrodictive arguments on the other, and these differences have some interesting consequences.

II

It would be an important achievement if we could state in precise terms a set of formal criteria for explanation and prediction. But this by itself may not help us much in understanding what explanation and prediction are all about in the first place; we may indeed say that the really significant thing is to gain an understanding of these scientific activities and procedures rather than merely to be able to state such criteria. Although this latter task is important, by itself it constitutes neither a necessary nor a sufficient condition for an adequate understanding of the concepts under consideration. Given such criteria, we would still want to know why they are required at all, what their point is, to begin with. In order to know this, we have to know and understand the point of giving explanations and making predictions.

To give an explanation is to give an argument of a sort; to explain is not merely to give additional bits of information; for these bits of information to be relevant in an explanation, they must explain whatever is to be explained. A request for an explanation is not necessarily a request for new information; new information is relevant only insofar as it explains. This is seen from the fact that mere possession of all the information necessary for an explanation of something does not necessarily mean that we have an explanation of it; in order to have an explanation, we

must perceive a certain relation, or connection, between what is to be explained and what explains it. To construe an explanation as a kind of argument is an essentially correct insight that is made explicit by the covering-law theory. But what kind of argument is an explanation? A covering-law explanation of course takes the form of deductive inference. Let us restrict ourselves to deductive inference here. What we shall say about it will apply, *mutatis mutandis*, to inductive or statistical inference as well.

Generally, then, what are we up to when we infer a statement from certain other statements? The answer to this question is simply that we may be doing a number of things; for all that we know, we may be doing exercises in logic or simply passing time. More seriously, however, there seem to be roughly three important types of activity any one or more of which may be involved in a given deductive inference: (1) most obviously, the inference may purport to *establish, substantiate, support,* or *prove the conclusion* on the basis of certain statements assumed as premises; (2) the inference may be intended to *establish* or *give support to one or more of the premises in an inductive manner;* (3) the inference may be intended merely to *exhibit logical relations obtaining between statements,* i.e., between the premises and the conclusion. When a scientific theory is proposed for acceptance on the ground that a number of well-established laws and confirmed predictions can be deduced from it, the point of the deduction is precisely (2). Examples falling under (3) abound in mathematics, as when the axiom of choice and the well-ordering theorem are shown to be logically equivalent by mutual deduction. Also in the empirical sciences we can find this kind of inference, as when two physical theories are proved to be essentially equivalent theories. Examples of inference falling under (1) are too obvious and familiar to cite here.

What then are explanatory arguments intended to show? One thing that an explanation does *not* purport to do is to *prove* or *establish* the conclusion of the argument; it does not purport to show that the event to be explained actually took place or is taking place. The following considerations seem to lend support to this contention.

First at least in some cases of explanation, the truth of the statements adduced as an explanation of an event is not nearly as certain as the fact that the event to be explained actually occurred or is occurring; in other words, often the explanandum statement is known with greater certainty than the statements of initial conditions or laws invoked in the explanans. Furthermore, we

often accept a law or theory simply on the ground that it explains. To view an explanatory argument as proving or substantiating the conclusion of the argument is incompatible with the usual hypothetico-deductive account of the confirmation of laws and theories. Especially, high-order laws and theories are said to derive their confirmation solely from their ability to imply, and thus explain, the low-order laws and theories. Therefore, we cannot claim *both* that high-order laws and theories establish or substantiate low-order ones by implying them *and* that the former receive confirmation from the latter also by implying them.

Secondly, suppose that explanations are construed as answers to certain types of questions, specifically, why-questions and how-questions. When we ask "Why do unsupported objects fall to the ground?", we *presuppose* or *presume* that unsupported objects do in fact fall to the ground; when we ask "How did Caesar die?", the question presupposes the truth of the statement that Caesar died. In most contexts, a question of the form "Why P?" or "How P?", where P is a statement, cannot be asked unless the asker knows or presumes that P,[3] and further, such questions cannot properly be answered unless P is true. This consideration shows that when we demand an explanation of an event, what we are asking for is not a proof or substantiation of the fact that the event occurred.

Thus, explanations cannot be viewed as inferences of the first type. Somewhat analogous arguments would rule out the second type also. In order to construct an explanation, we of course have to ascertain certain initial conditions and establish laws, but this is only a necessary preliminary step in constructing an explanation; it is not the same thing as explaining any more than collecting paints and preparing canvases are the same as painting. Explaining comes after such preparatory steps.

By a process of elimination, we are left with type (3). Among the three uses of inferential argument considered here, this last type appears to come closest to explanations. To be sure, in an explanation we are not interested in the logical relations obtaining between statements as such; but, by establishing them, we show that the events they describe are also connected in a certain way. And according to the covering-law theory, this connection is mediated by the laws used in the explanans; thus, we may speak of *nomological connections* here. If the covering-law theory is correct in its basic tenets, these nomological connections are essential

[3] This does not hold in some cases; consider, for example, "Why should I apologize?" and "How should I know?"

for explanations of particular events and states as well as for explanations of general regularities.

What of predictions and retrodictions? In contradistinction to explanations, they *are* attempts at gaining knowledge of particular events and states; when we explain, we are not trying to increase the stock of our knowledge of specific events and states (although we may have to acquire such knowledge in order to explain); but this is precisely what predictions and retrodictions attempt to accomplish. Basically, they are projections from the known data to the unknown, and needless to say, the job of a predictive or retrodictive argument is to *substantiate,* or *give support to,* such a projection. When an argument of the covering-law type is used for such purposes, the premises (i.e., the laws and statements of initial conditions) are what justifies our claims to know future or past events and states; thus they function as *evidence* for the conclusion which expresses a prediction or retrodiction.[4] This contrasts rather sharply with the fact that when a covering-law argument is used as an explanation, the relation between the premises (i.e., the explanans) and the conclusion (i.e., the explanandum) cannot be construed as an evidential relation. We may sum up by saying that when a covering-law argument is used as an explanation, the conclusion of the argument is *evidentially* or *epistemically independent* of the premises, whereas in a predictive or retrodictive argument of the covering-law variety, the conclusion is necessarily dependent on the premises for evidential support.

The above seems to be the fundamental difference of rationale between explanations on the one hand and predictive and retrodictive arguments on the other. Explanations are attempts to exhibit nomological connections between events and states and show how they fall into lawful patterns, and on a psychological level, the notion of understanding is an essential ingredient of the concept of explanation. When we predict or retrodict, we are seeking to acquire more knowledge of specific, concrete events and occurrences, and in giving a predictive or retrodictive argu-

[4] There is a related sense of 'prediction' for which this is not strictly true, i.e., the sense in which we *make predictions* in order to confirm some hypothesis. In this sort of case, we do not categorically assert predictions because by the nature of the case the hypothesis which, in conjunction with some singular statements, entails them is only hypothetical and tentative. Nor are we intrinsically interested in the events "predicted" in this sense; our main interest lies with the hypothesis. Predictions of this type are best regarded as cases of type (2) inference. Obviously, there are retrodictions of the same type.

ment, we are seeking to substantiate our claims to such knowledge; understanding seems to have no place in predictions or retrodictions.

One consequence of the foregoing discussion is this. In a predictive or retrodictive argument, the appeal to general laws is not essential; if the primary function of the argument is to establish or prove the conclusion, then the fact that the argument is nomological, i.e., makes essential use of laws, is inessential to the usefulness or reliability of the argument. Although the conformity of a predictive or retrodictive argument to the requirements of the covering-law model is sufficient, it is not necessary. Thus, the covering-law model can be used for predictive and retrodictive purposes, but the acceptability of a predictive or retrodictive argument is not lessened merely because it is not a covering-law argument. Statements of restricted regularities would suffice for such purposes, and the so-called accidental generalizations can function with dignity in projective arguments. For example, the predictions of the occurrences of high and low tide at a particular point on the earth can properly be based on their regular connection with the phases of the moon without invoking Newton's theory of gravity.

III

Philosophers have noted the existence of explanations that lack "predictive power," such as explanations of the occurrence of earthquakes and evolutionary explanations of the emergence of a new biological species. It is hardly possible, at the present state of scientific knowledge, to predict these events with any significant reliability; and this fact has led some philosophers to condemn these explanations as "circular" and "pseudo-explanations."[5] In this section, we shall inquire why these explanations are not fit for predictive purposes, and show how they can be vindicated as *bona fide* scientific explanations on the basis of our discussion in the preceding part.

First, what does it mean to say that an explanation has predictive power? The common usage of most writers on this subject seems to be this: suppose that the event to be explained is to occur at time t. Then, to say that a given explanation of the event has predictive power is to say that if the initial conditions of the explanation are ascertained before t and the laws used in the explanation are also known before t, then an argument-token which is of the same type as the explanation could have functioned as a predictive argument to predict the occurrence of the event. When understood

[5] For instance, see John G. Kemeny, *A Philosopher Looks at Science* (Princeton, N.J.: D. Van Nostrand, 1959), p. 157, pp. 160–161.

this way, the claim that every covering-law explanation has predictive power is obviously and trivially true.[6] But this should not lead us to believe that a given explanatory argument could in fact have functioned as a predictive argument; any covering-law explanation can *if* the initial conditions can be ascertained prior to the occurrence of the event to be explained and *independently of* the explanandum statement. But this is not always the case.

When an explanatory question is posed or the request for an explanation is made, the explanandum statement is "given"; in most cases, when we ask "Why *P*?" or say "Explain why *P*," the statement *P* is implicitly asserted. To answer such a question or meet such a request by giving a covering-law explanation, we must provide relevant laws and initial-condition statements. But how are these relevant initial-condition statements obtained? In some cases, we may be able to draw these singular statements from the accepted body of knowledge of specific events and states; in other cases, they may have to be ascertained after the request for an explanation is made—we look for them for the specific purpose of explaining the given event. But in these latter cases, our knowledge that the event to be explained actually occurred may play an evidentially essential role in ascertaining some or all of the initial-condition statements to be used in the explanans. Even though the explanandum statement and the initial-condition statements are logically independent of each other, the former may confer a more or less high degree of confirmation on the latter by virtue of some empirical generalizations, or the former may even logically entail the latter when taken in conjunction with some available universal laws.

It is clear, then, why these explanations have no actual predictive power. The initial conditions cannot be ascertained before we know that the event to be explained actually occurred. But as we saw earlier, the essential feature of a predictive argument is the evidential dependence of the conclusion upon its premises;

[6] However, the converse of this claim is false, i.e., the claim that any given covering-law predictive argument can be converted into a covering-law explanation. To see why this claim is false, it suffices to realize that a covering-law argument whose singular premises describe events that are temporally later than the event described by its conclusion do not normally have an explanatory use, whereas such an argument could be used for prediction and retrodiction, provided that the singular premises can be ascertained by independent arguments. This is where the thesis of the structural identity between explanations and predictive arguments really breaks down. The customary objection against the thesis on the basis of the alleged fact that our ability to explain does not match our ability to predict and vice versa has no force and seems to rest on a misunderstanding of the thesis.

the premises are "epistemically prior" to the conclusion. But in explanations of the type just described, some of the premises are evidentially dependent upon the conclusion; the conclusion is "epistemically prior" to the singular premises of the argument.

An example of *ex post facto* explanation, which is a legitimate scientific explanation by any reasonable standard, is the following. Given the phenotypical features of one generation of pea plants, we cannot categorically predict, on the basis of the Mendelian laws, the phenotypical features of the second generation. This is due to the fact that the Mendelian laws do not provide a means by which the allele structure of the genes of peas can be determined from the phenotypical features of the peas and this information is essential for the desired prediction. However, we can explain genetically the phenotypical characteristics of the second generation after we have ascertained them, i.e., after the second generation has come into being. For under suitable circumstances, the allele structure of the genes of the first generation can be determined on the basis of the phenotypical features of *both* the first *and* the second generations. The Mendelian laws are well-established empirical laws with explanatory and predictive power; but, for the situation described here, they can explain a phenomenon that they could not have predicted before the phenomenon actually occurred.

It is important to notice that the evidential dependence of the statements of initial conditions upon the explanandum statement is a contingent fact—contingent on the state of knowledge and technology and also on the knowledge and competence of the explainer. Since the explanandum statement and the statements of initial conditions of covering-law arguments are to be mutually independent logically,[7] the question whether the latter can be ascertained independently of the former is largely a practical matter.

Are *ex post facto* explanations "circular" and "pseudo-explanations," as some philosophers claim? One of the conditions Kemeny lays down for "good explanation" is that the initial conditions must be known independently of the event to be explained.[8] *Ex post facto* explanations in our sense clearly violate this condition; in fact, we defined them precisely on the basis of this violation. It seems that Kemeny's charge is quite wrong and arises from a misunderstanding of the basic rationale of explanatory arguments.

[7] See the author's "On the Logical Conditions of Deductive Explanation," *Philosophy of Science*, **30** (1963).

[8] Kemeny, *loc. cit.*

Scientific explanations take the form of an argument or inference, but as we emphasized earlier, this does not mean that explanations are inferences that purport to establish or prove their conclusions, i.e., the explanandum statements. If explanations were construed as arguments with such intent, then *ex post facto* explanations would involve vicious circularity. But, as we suggested earlier, such a construal of explanatory arguments is a mistake. Explanations are basically attempts to systematize *known* events and states. More generally, the charge of vicious circularity is appropriate only if an argument purports to prove or establish its conclusion; since explanations are not arguments with such intent, the charge of circularity is misplaced. We saw earlier that the evidential independence of the explanandum statement from the explanans is a necessary feature of explanations; but there seems to be no good reason to require that the explanans be evidentially independent of the explanandum statement as well. Not only would this requirement be contrary to the actual practice of the scientist; but also, no logical and methodological consideration would seem to justify it. *Ex post facto* explanations are legitimate scientific explanations; they lack practical predictive power because of the peculiar evidential relation between the initial-condition statements and the explanandum statement, but this is no reason to incriminate them *as explanations.* When an *ex post facto* explanation is unacceptable, the reason is often simply that the explanatory premises established *ex post facto* are not adequately confirmed, their sole evidence being the explanandum statement.

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COMMENTS AND CRITICISM

KNOWLEDGE AND PROBABILITY

IN a recent article,* Professor Sleigh has shown that certain principles that Professor Chisholm accepts in his book, *Perceiving,* lead to contradiction. Two of the principles in question (cf. 217) are

P1. *S* has adequate evidence for *h* if and only if *h* is more probable than not on the total evidence of *S*.

* R. C. Sleigh, Jr., "A Note on Some Epistemic Principles of Chisholm and Martin," this JOURNAL, **61**, 7 (Mar. 26, 1964): 216–218.

and

P2. If S has adequate evidence for i and S has adequate evidence for j, then S has adequate evidence for i and j.

Sleigh suggests that we reject P2 to avoid the contradiction in question.[1] I wish to argue that, as Chisholm is using the locution 'S has adequate evidence for h', we will not be able to avoid contradiction by rejecting P2, and, consequently, that we must reject P1. Finally, I shall argue that no matter how high we raise the degree of probability, provided it is less than 1, we will still be led to contradiction. Thus, we cannot equate having adequate evidence with any degree of probability less than 1.

The two principles mentioned above lead to contradiction in virtue of the Special Multiplication Rule.[2] According to this rule, if i and j are independent, then the probability of i and j on evidence e equals the probability of i on evidence e times the probability of j on evidence e. As a result of this rule, in some cases it will be true that

(i) i is more probable than not on the total evidence of S

and that

(ii) j is more probable than not on the total evidence of S

but false that

(iii) The conjunction of i and j is more probable than not on the total evidence of S

For the probability of the conjunction on the total evidence of S is equal to the probability of i on the total evidence of S times the probability of j on the total evidence of S. Consequently, the probability of the conjunction may be less than $\frac{1}{2}$ when the probability of each of the conjuncts is greater than $\frac{1}{2}$ with respect to the same evidence.

Of course, it follows from P1 and the falsity of (iii) that

(iv) S has adequate evidence for i and j

is false. On the other hand, it follows from P1, P2, (i), and (ii) that (iv) is true. To avoid this contradiction, Sleigh suggests that we reject P2. I wish to argue that even if we do reject P2 the acceptance of P1 will still lead to contradiction. Consequently, I believe that we must reject P1, a somewhat more radical alternative than the one Sleigh proposes.

[1] Page 218. Sleigh also suggests that we reject the following principle: if i or j is acceptable for S, then either i is acceptable for S or j is acceptable for S. For the purposes of my paper, we may accept this suggestion.

[2] Sleigh does not appeal to this rule in his article.

As Chisholm is using the locution '*S* has adequate evidence for *h*', the following is to be accepted as a definition of knowledge:

D1. *S* knows *h* if and only if *S* accepts *h*, *S* has adequate evidence for *h*, and *h* is true.[3]

I shall now prove that a contradiction will result from the acceptance of P1 even if we reject P2. The proof will involve D1 and another principle to be introduced later. To obtain the contradiction, suppose that

(v) *S* accepts *i*
(vi) *S* accepts *j*
(vii) *S* accepts *i* and *j*

and

(viii) *i* and *j* are both true

As we have seen, we may suppose that *i* and *j* are such that (i) and (ii) are both true and (iii) false. In that case, it will follow from P1 that

(ix) *S* has adequate evidence for *i*

and that

(x) *S* has adequate evidence for *j*

Given D1, it follows from (v), (viii), and (ix) that

(xi) *S* knows *i*

and it follows from (vi), (viii), and (x) that

(xii) *S* knows *j*

As we noted earlier, it follows from P1 and the falsity of (iii) that

(iv) *S* has adequate evidence for *i* and *j*

is false. Moreover, given D1, it follows from the falsity of (iv) that

(xiii) *S* knows *i* and *j*

is also false.

If Chisholm were to accept the following principle:

P3. If *S* knows *i* and *S* knows *j*, then *S* knows *i* and *j*.

the preceding results would yield a contradiction. However, following a suggestion of Pseudo-Scotus, Chisholm suggests that perhaps the principle should be rejected because a person who knows

[3] R. M. Chisholm, *Perceiving: A Philosophical Study* (Ithaca, N. Y.: Cornell Univ. Press, 1957), p. 16.

i and knows j might fail to "put two and two together."[4] Even if we regard this as decisive against P3, we can easily modify the principle to meet this objection by adding to the antecedent of the principle the qualification that the person does "put two and two together," that is, that he accepts i and j. We would then have the following principle:

P4. If S knows i and S knows j and S accepts i and j, then S knows i and j.

This principle should be acceptable to Chisholm, but from it we can derive a contradiction from our earlier results. For it follows from (xi), (xii), (vii), and P4 that

(xiii) S knows i and j

is true. Therefore, it follows from P1, P4, and D1, together with a perfectly consistent set of suppositions, that (xiii) is both true and false. To avoid this contradiction, I suggest that we reject P1.[5]

Finally, I wish to argue that it will not be possible to find an acceptable alternative to P1 by simply raising the required degree of probability. To see this, suppose that the fraction m/n is any fraction less than 1. The principle:

P5. S has adequate evidence for h if and only if h is probable to degree m/n on the total evidence of S.

will lead to a contradiction just as P1 did. Again this is a consequence of the Special Multiplication Rule. For if i and j are independent and if i is probable to the degree m/n on the total evidence of S and j is probable to the degree m/n on the total evidence of S, then the conjunction of i and j will be probable to the degree m/n times m/n on the total evidence of S. The latter degree of probability will be less than m/n. Therefore, we would be able to use P5 in place of P1 to prove that

(ix) S has adequate evidence for i

and

(x) S has adequate evidence for j

are both true in some case in which

(iv) S has adequate evidence for i and j

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[4] R. M. Chisholm, "The Logic of Knowing," this JOURNAL, **60**, 25 (Dec. 5, 1963): 775.

[5] This argument was suggested to me by a similar argument presented by Herbert Heidelberger, "Knowledge, Certainty and Probability," *Inquiry*, **6** (1963): 245. Some of the other ideas in this paper emerged from a discussion with Professor Heidelberger and a discussion with Professor Chisholm.

is false. We would then be able to derive a contradiction by using D1 and P4. Therefore, I conclude that adequate evidence cannot be equated with any degree of probability less than 1. The question that remains open is whether adequate evidence can be equated with a degree of probability equal to 1.

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BOOK REVIEW

The Concept of Method, JUSTUS BUCHLER. New York and London: Columbia University Press, 1961. viii, 180 p. $4.00.

Though much has been written about methods of one kind or another, according to the preface of Professor Buchler's book, little has been written about the concept of method, or about what makes a method methodic. The purpose of the book is to augment the literature of this more general problem by extending in relevant directions the metaphysical system begun in two of the author's earlier books.[1] But though the present work is in a sense "one elaborate chapter within the larger structure," it is "designed as a separately readable essay." Instead of the larger structure, the author has "freely chosen" what he calls "polemical contexts" within which to exhibit the concept of method. These contexts include—once the enterprise has got started by a brief consideration of a definition of Morris Cohen's ("any procedure which applies some rational order or systematic pattern to diverse objects") —Bentham, Coleridge, Descartes, Dewey, and Whitehead.

If by 'freely chosen' Buchler means what he would by 'chosen at random', then the choices of Bentham and Coleridge were lucky accidents, allowing him as they do to set up a scale of which they represent extreme positions.

> . . . the contrast between Bentham and Coleridge, whose theories of method were composed at about the same time, reflects in vivid form the kind of contrast in conceptual emphasis that has been held representative of the Enlightenment and Romanticism. Grouping and predictable structure [redacted] against movement and life; appeal to the tested and tried as against inspiration and novelty; love of uniformity as against excitement over "the pulse that throbs"; strategic remedy as against growth and uniqueness (46).

The contrast, when used to distinguish theories of method from one another, is in important respects like a contrast familiar in recent decades, between contexts (or "the context") of justification

[1] *Toward a General Theory of Human Judgment,* 1951, and *Nature and Judgment,* 1955.

or explication and those of discovery. For Bentham method is "the strategic dissemination of prudence" (19) and is essentially remedial in nature. It employs what Bentham calls the tactic faculty, but the tactics (to borrow an apt pair of metaphors from Ryle) are those of parade-ground drill and not guerilla warfare. What is missing from Bentham's account of method is invention. Invention is not missing from Coleridge's account, however; on Coleridge's account method and invention are one. Method for Coleridge is to be distinguished from "mere orderliness" and "dead arrangement"; method has a "unifying principle of life" and a "purpose in view" which are supplied by its leading idea, and this idea rests ultimately on the will. This is a conception of method suitable more for contexts of discovery and invention than for those of systematization; it makes of method, as Buchler points out, a eulogistic concept. "Considering the traits that attract him, it is always a fair question whether [Coleridge] is legislating the nature of method or consenting to discover it" (54).

Descartes is interested more in discovery than in justification, in finding new truths than in rearranging old, and to this extent he is like Coleridge. But in wanting to govern method by rule and not by inspiration he is like Bentham. Dewey too places heavy reliance on rule, but for him the dependence is reciprocal; the rule is not infallible, and it is subject to modification in the light of the results of its own applications. Where Dewey appears on the Coleridge-Bentham scale is not clear, but his willingness to supersede the rule suggests that he is on the Coleridge, or left, side of Descartes. However, Buchler seems now to have lost interest in Bentham and Coleridge and to be treating Descartes and Dewey as the extremes. "In the Cartesian theory [discretion] is at best a necessary evil, a prop, lurking outside the proper domain of method. At the other extreme, in the Deweyan theory, it is inflated into intelligence, as if there were no other methodic factors determining its function" (140). Perhaps Bentham and Coleridge are too far out to be taken seriously. Whitehead too is beyond the pale for Buchler. Like Dewey, Whitehead believes that rules are incapable of governing method without assistance, but Whitehead and Dewey turn in different directions for that assistance. Whitehead's distinction between practical reason and speculative reason is very nearly Dewey's distinction between intelligence and intellect, but whereas Dewey repudiates the second member of each pair and turns to the first, Whitehead goes the other way, and goes much further. For him,

> . . . method should be sought and pursued up to a certain point, beyond which something more fundamental, more fundamentally human, must be encouraged to thrive in complete freedom. . . . Speculative reason, seeking "the

general reasons beyond limited reasons,'' is essentially ''untrammeled by method.'' It seeks to ''understand all methods . . . by transcending all method''. . . . We ''now speak of the speculative Reason in the place of Inspiration'' (161, 156).

Though Coleridge's inspiration seems at least as inimical to method as Whitehead's speculative reason, Coleridge, unlike Whitehead, continued to take the method seriously. Whitehead is method acting; for Coleridge there really is a method, yet there's madness in it.

One would be wrong to suppose that *The Concept of Method* is simply a discussion of the views of Bentham *et al.* about method. Buchler is, as he says, using these views as polemical contexts. Nor should one suppose that within these contexts Buchler is concerned entirely with the question at issue in the Bentham-Coleridge (or is it now Bentham-Whitehead?) scale. He does discuss at some length the concept of a rule (even saying some Wittgensteinian things about it in a very un-Wittgensteinian way), but he discusses any number of other concepts as well. To mention just a few not already mentioned, method is more or less sharply distinguished from—or in some other way paired for the sake of discussion with—arrangement, art, decision, estimation, groping, habit, inquiry, knowledge, means, operation, order, plan, power, procedure, purpose, query, system, technique, unity, vagueness, and way, not to mention methodography (the reflection on method of the artist or the diplomat, as contrasted with the scientist, whose reflection is for the sake of normative advice), methodology (which bears to method approximately the relation that historiography bears to history and, as seen in the loose way the word is used, is open to the same misinterpretation—though whereas 'history' usually swallows up 'historiography', it is 'methodology' that swallows up 'method'), and methodolatry (''Men afflicted with methodolatry become self-righteous, and in their euphoria fancy themselves to have acquired unsuspected health'' [106]). These may seem to be a lot of things to talk about in connection with method, but we are told in the preface that the book ''by no means contains all that I have to say about its subject.'' Nevertheless, it contains enough to provide a basis for Buchler's own definition: ''A method is a power of manipulating natural complexes, purposively and recognizably, within a reproducible order of utterance; and methodic activity is the translation of such a power into the pursuit of an end—an end implied by the reproduction'' (135). The terms in the definition are explained in the pages following it (for instance, ''By an 'order of utterance' we intend a 'perspective of utterance' '' [137]); the explanation uses terms which are themselves explained elsewhere.

The book has a wealth of suggestions for methodographers,

methodologists, and philosophers of method, and it should have a salutary effect on methodolaters as well. The writing is methodic, but the reader who stays with it will be rewarded by sudden insights, flashes of wit, revealing metaphors, and suggestive comparisons. I have two criticisms. The first is that the book is deprived of substance by being too episodic; themes are continually introduced and left undeveloped. Examples are the question of how logic is related to method and the much-neglected problem of the relation of ethics to logic and to method; both are introduced on page twelve, both seem to me to be of first importance, but unlike Topsy neither grows. It may be that these and other concepts introduced are so interconnected that what is said of one applies to several, but then what is needed is to make the interconnections clear. The second criticism is that although I am willing to suppose that the concept of method (as opposed to methods, or a method) deserves a book all to itself, I am not sure why. As far as I can see, whatever is said in this book with the help of the concept of method could be said without it—the plethora of other concepts introduced suggests this—and if, as the last few pages of the book indicate, there is something important to be said about philosophy that can be said only in terms of method, this fact needs to be shown. Though whoever reads the book will profit from it, I suspect its permanent value will be the value it derives from contributing to Buchler's general system.

ALAN PASCH

UNIVERSITY OF MARYLAND

NOTES AND NEWS

The Department of Philosophy at the State University of New York at Buffalo (formerly the University of Buffalo) is pleased to announce the addition of seven new members for 1964–65: Marvin Farber, Distinguished Service Professor; Edward H. Madden, Professor; Berkley Eddins, Assistant Professor; Lynd Forguson, Assistant Professor; John T. Kearns, Assistant Professor; James Pratt, Assistant Professor; and Calvin Rand, Lecturer.

As of June, 1964, the journal *Philosophy and Phenomenological Research,* edited by Prof. Farber, will be transferred to the new address.

The Department of Philosophy of Dickinson College is pleased to announce that Prof. Eva Berczeller will be Visiting Associate Professor for the year 1964–65. Professor Frederick Ferré will be on leave as Fellow of the Graduate Council of the Humanities at

Southern Methodist University. Dr. George Allan will be Acting Chairman.

Professor Albert William Levi of Washington University, St. Louis, will lecture at the University of Graz, Austria, during the academic year 1964–5, on intellectual history and the philosophy of literature.

Paul Weiss will retire as Editor of *The Review of Metaphysics* in June, 1964. Richard J. Bernstein will be the Editor, beginning with the issue of September, 1964. Walter G. Emge will be in charge of the *Summaries and Comments* section of the *Review*.

The *Monist* announces the topics of its issues to be published in 1965: **49**, 1, Linguistic Analysis and Phenomenology; **49**, 2, Agent and Spectator; **49**, 3, Philosophy of Law; **49**, 4, John Duns Scotus; and in 1966: **50**, 1, Concept of Existence; **50**, 2, Reasons in Art Criticism; **50**, 3, Philosophy of Plato; and **50**, 4, Symbol and Myth. Scholars who wish to submit papers should communicate with the editor well in advance.

The Twenty-Fourth Annual Meeting of the Virginia Philosophical Association will be held at Virginia State College, Petersburg, Virginia, on October 15–16, 1964. Among the papers to be read will be the following: "Conceptual Consideration in Ethics," by Norman L. Glover, Virginia Polytechnic Institute; "The Fallacy of Excluded Ends," by George B. Thomas, Jr., University of Virginia; and "Some Explanations of Explanation," by Donald Rogers, College of William and Mary.

The Society for Religion in Higher Education is offering a number of post-doctoral fellowships to encourage the growing interest in the relation of scholarship in religion to scholarship in other fields of study. These fellowships are made possible through a grant from The Danforth Foundation.

Grants are available to: (1) scholars in the humanities, social sciences, and natural sciences for a year of study in religion, and (2) scholars in religion for a year of study in another discipline; they are open to scholars in any institution of higher learning in the United States or Canada, without respect to religious affiliation or non-affiliation, who have a Ph.D. degree or its equivalent plus at least three years of teaching experience.

Further information may be secured from: Lawrence P. DeBoer, Executive Director, The Society for Religion in Higher Education, 400 Prospect Street, New Haven, Conn.

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